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### Journal Item

#### How to cite:

Chimisso, Cristina (2003). The tribunal of philosophy and its norms: History and philosophy in Georges Canguilhem's historical epistemology. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 34(2) pp. 297–327.

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Version: Accepted Manuscript

Link(s) to article on publisher's website:

[http://dx.doi.org/doi:10.1016/S1369-8486\(03\)00027-X](http://dx.doi.org/doi:10.1016/S1369-8486(03)00027-X)

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# The tribunal of philosophy and its norms: history and philosophy in Georges Canguilhem's historical epistemology

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Received 15 June 2002; received in revised form 23 December 2002

## Abstract

In this article I assess Georges Canguilhem's historical epistemology with both theoretical and historical questions in mind. From a theoretical point of view, I am concerned with the relation between history and philosophy, and in particular with the philosophical assumptions and external norms that are involved in history writing. Moreover, I am concerned with the role that history can play in the understanding and evaluation of philosophical concepts. From a historical point of view, I regard historical epistemology, as developed by Gaston Bachelard and Georges Canguilhem, as a conception and practice which came out of the project, elaborated in France from the 1920s to the 1940s, of combining history of science and philosophy. I analyse in particular Canguilhem's epistemology in his theory and practice of history of science. What he called 'normative history' is the focus of my analysis. I evaluate the question of the nature and provenience of the norm employed in normative history, and I compare it with the norm as discussed by Canguilhem in *Le normal et le pathologique*. While I am critical of Canguilhem's treatment of history, I conclude that his philosophical suggestion to analyse the formation of scientific concepts 'from below' represents a useful model for history and philosophy of science, and that it can be very profitably extended to philosophical concepts. © 2003 Elsevier Science Ltd. All rights reserved.

**Keywords:** Canguilhem; Bachelard; Koyré; Historiography; Normativity; Historical epistemology

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## 1. Introduction

The relation of philosophy with history has often been a difficult one. The great majority of philosophers who are included in the ‘canons’ and in most compendia of the history of philosophy appear not to have been keen on historical research. Before the eighteenth century, philosophical reflection hardly ever engaged with history. Enlightenment philosophers did take history as the core of their investigation but, often, as in the case of Condorcet, Turgot and Rousseau, their histories were highly philosophical, and were little concerned with the construction of small-scale narratives through original sources, or the establishment of particular events. Many philosophers have engaged with the history of their own discipline; but they have largely seen it as a succession of ideas and thinkers sitting on their own time arrow, undisturbed by external events. Although this picture may be readily accepted by those philosophers (and historians) who do not consider the relationship between philosophy and history a fruitful one, it is, if not false, at least incomplete: in fact many philosophers have engaged with history, including several whom I am going to discuss in this paper. This picture may also be unduly simplified, as the relation between history and philosophy has assumed different forms and has played different roles in different institutional settings and national traditions. In many quarters this relation still seems to remain unresolved, or unwelcome. In some philosophical traditions still alive, notably the analytical tradition, history has been seen as irrelevant, indeed as the opposite of what philosophy is about: history is about contingency and change, while philosophy is about necessity and timeless truths.

In this context, the notion of historical epistemology may be seen as immediately striking by some, for history and philosophy appear to be securely interlocked. To make things more intriguing, the history in question is history of science rather than philosophy. Recently, Lorraine Daston and Jürgen Renn have described their research programmes as historical epistemology (Daston, 1994, 1997; Renn, 1995). I shall not be concerned here with their programmes, which share the name but not the methods and aims with the doctrines that I am going to discuss. I shall concentrate on the emergence of historical epistemology as a new approach to the study of scientific knowledge at the hands of Gaston Bachelard and Georges Canguilhem. My focus will be in particular Georges Canguilhem’s historical epistemology, and the type of history that he practised and defended in his work. He called his style of history ‘normative history’, that is, a narrative constructed by assuming a norm that allows one to evaluate and judge past doctrines. Normative history and historical epistemology are indeed two sides of the same type of project. Gaston Bachelard was Canguilhem’s recognised point of reference in this type of enterprise; for this reason comparisons between their projects will run throughout this article.

The question on which I shall focus in particular is the origin and nature of the norm employed in order to judge concepts and theories. Canguilhem’s work is extremely interesting in this regard, because not only did he write normative histories and defend this practice in several articles, but he also wrote a history of the concept of the norm in his *Le normal et la pathologique*. Is the ‘norm’ as conceived in *Le normal et la pathologique* the same as the norm employed in the construction of

normative histories? Does it have the same origin? These will be the main questions I shall address with regard to the norm in Canguilhem's work.

In sympathy with the topic of this article—and the convictions of its author—my treatment will be not only theoretical but also historical. I shall discuss Canguilhem's conception and practice of history and philosophy in their interaction, and situate them in the debates which took place among French scholars in the period before and during Canguilhem's career. My view of historical epistemology is as a conception and practice which came out of the project of combining history of science and philosophy which had been elaborated from the 1920s to the 1940s in Paris. This approach favoured both a philological attention to texts and a consideration of science within its cultural and social context, and at the same time addressed philosophical questions about the mind and knowledge. The relation between historical epistemology and the original project of combining history of science and philosophy is not one of simple derivation, for historical epistemology partly abandoned one of the main aspirations of that project, namely a detailed and comprehensive historical account of past doctrines. My version of the historical place of Bachelard's and Canguilhem's projects, and their relationship with the contemporary historiography, is rather different from most of the current ones, notably from that of Michel Foucault.

## 2. Histories of the history of the sciences

In the *Archaeology of knowledge*, Michel Foucault provides a synthetic account of recent historiography of the sciences. He distinguishes three types of history of the sciences: that which employs a 'recurrential analysis',<sup>1</sup> epistemological history and archaeological history. The first type for Foucault belongs to mathematics, and consists in reinterpreting past theories so as to make them particular cases of current theories. Although Foucault only cites Michel Serres, the concept of recurrent history was elaborated by Bachelard in essentially the same terms as Foucault's. Unlike the other two types of history, this history is an integral part of science itself. The second type of history of science is for Foucault epistemological, and it is exemplified by the works of Gaston Bachelard and Georges Canguilhem. Foucault describes it as:

A type of historical analysis . . . [that] takes as its norm the fully constituted science; the history that it recounts is necessarily concerned with opposition of truth and error, the rational and the irrational, the obstacle and fecundity, purity and impurity, the scientific and non-scientific. It is an *epistemological* history of science. (Foucault, 1972, p. 190)

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<sup>1</sup> 'Recurrential analysis' is the expression used in the English translation of *The archaeology of knowledge* (Foucault, 1972, p. 190). It translates the French 'analyse récurrentielle' (Foucault, 1996 [1969], p. 248).

The third type is Foucault's own archaeology. Archaeology abandons the normative point of view and, as a consequence, the concern with the distinction between scientific and non-scientific forms of knowledge. In fact, Foucault's archaeology, in his own description, is aimed to uncover 'descriptive practices in so far as they give rise to a corpus of knowledge, in so far as they assume the status and role of a science' (Foucault, 1972, p. 190).

Foucault's classification of the historiographies of science is useful as an analytical tool and as a starting point of a reflection on the aims of different types of history of science. One can't hide from the fact, though, that Foucault's is a rather recurrent reconstruction of the history of historiography. Just as the sciences, in Bachelard and Foucault's view, reconstruct their history in a rational manner from the point of view of the present, and condemn to oblivion those theories and practices that cannot be integrated in this history, so Foucault's account of the historiography of science is simplified and progressive. It is progressive in that his three types of historiography are in order of increasing depth of analysis and reflexivity. Recurrent history is unreflexively normative: it does have a norm of scientificity, which coincides with the norm of current science, but this norm is unreflexively assumed. As a consequence its historicity is hidden, and scientificity can be anachronistically projected back to past theories and practices, which are reinterpreted and reconstructed so that they conform to the norm.

Epistemological history is reflexively normative, for it consciously employs current science as a norm to judge knowledge, and, unlike recurrent history, it is able to analyse and determine when the leap between non-scientific and scientific discourses occurs. Epistemological history understands the mechanism which produces recurrent and lapsed history and understands how both non-science and science are produced. This type of history does not de-historicize current science; indeed, although it does take current science as its norm, it nevertheless regards its norm as the result of an open-ended historical process.

Finally, Foucault presents his archaeology as a step forward, posing itself at a deeper and more general level than epistemological history. Archaeology is reflexive without normatively assuming the point of view of current science. Its analyses of discursive practices apply to alchemy as well chemistry, to sciences as well as humanities and crucially to history and philosophy, potentially including the first two types of history of science. Moreover, the current 'norm' undergoes the same analysis as any other discursive practice.

Foucault's account of the history of history of science outlined above is recurrent also in what it leaves out. Bachelard and Canguilhem's normative approach to history of science was by no means the only alternative to an unreflexive, 'recurrent' history of science. Needless to say, the historiography of the sciences had a much more troubled history than a brief outline could show. In particular, Foucault leaves out the historiographical trends that took the historical craft very seriously, and regarded an exhaustive historical reconstruction as necessary for providing answers to philosophical questions about the mind and knowledge. It is not just that there have been many approaches to the study of the history of science, of which Bachelard's and Canguilhem's represent two related types, united by their normative character. His-

torical epistemology also stands in a close historical relationship with other approaches, in particular that of studying history of science as part of epistemology, which was promoted by philosophers from the 1920s onwards.

An important centre for the elaboration of the project of the combination of history and philosophy of science was the Institut d'Histoire des Sciences et Techniques, founded in 1932 by Abel Rey, professor of history and philosophy of the sciences at the Sorbonne, who was succeeded on the directorship and chair by Bachelard in 1940, and then by Canguilhem in 1955. As conceived by Abel Rey, the ideal of the Institut d'Histoire des Sciences et Techniques was the fusion of history and philosophy: for him, as for other contemporary philosophers, the philosophical study of the mind, and epistemology, required a comprehensive historical study and a careful analysis of sources. The history of the sciences as taught in the 1920s in science faculties, though, was of no use to philosophers. A new history of the sciences was needed. This new history was to be 'general', as Abel Rey argued in one of his talks on the methods and aims of the discipline (Rey, 1935, pp. 34–49). The syllabuses of the courses offered at the Institut d'Histoire des Sciences et Techniques reflected this broad approach to history of the sciences: they spanned from history of physics to history of sociology, and from antiquity up to the present day.<sup>2</sup> This approach was not just the expression of the French educational ideal of *culture générale*. Philosophers such as the director Abel Rey sought to investigate the mentality behind the cultural production of a certain period, expressed by the various sciences.<sup>3</sup> At the same time, this study required punctilious examination of sources and an exhaustive account of the wider culture and society in which the sciences under study had developed. The historian and philosopher of science Hélène Metzger, associated with the Institut d'Histoire des Sciences et Techniques, thus illustrated this point in the field of history of chemistry:

... no point of human history is irrelevant for the history of the sciences; social and political history, history of philosophy, history of literature including history of theatre ... , history of industry and trade, to which could be also added history of art and history of occultism, are [all] useful to the researcher who proposes to study the chemists' writings. (Metzger, 1935a, pp. 163–164)

What Metzger was expressing here is the ideal of total history, which was promoted by historians and philosophers of science, some of whom, like herself and Abel Rey, belonged at the same time to the Institut d'Histoire des Sciences et Techniques and

<sup>2</sup> See *Thalès*, 1 (1934), pp. vi–viii; 2 (1935), pp. i–iii; and following years.

<sup>3</sup> The word 'mentality' had been introduced by Lucien Lévy-Bruhl, historian of philosophy, founder of the Institute of Ethnology at the Sorbonne, and author of *La mentalité primitive* (Lévy-Bruhl, 1922). The term quickly spread to other disciplines, including philosophy, history, psychology, pedagogy and sociology.

the Centre de synthèse historique—directed by Henri Berr—the aim of which was precisely the development of total history.<sup>4</sup>

Far from being a simple opposition to recurrent history, as Foucault's account suggests, the work of Georges Canguilhem represents a very important splinter resulting from the disintegration of the ideal of combining total history, philological analysis of texts and philosophical investigation. This ideal was perhaps just too ambitious, and the result was that students of history of science eventually renounced either one or another part of the programme. Michel Fichant attributes to Gaston Bachelard and Jean Cavaillès (1903–1944),<sup>5</sup> the development of historical epistemology, to Alexandre Koyré the practice of epistemological history, and to Georges Canguilhem that of the epistemology of the biological sciences, based on a Bachelardian history of science (Fichant, 1973, pp. 168–171). We can see here two of the products—although related ones—of the disintegration of the ideal: historical epistemology on one side (Bachelard, Cavaillès, Canguilhem) and epistemological history on the other (Koyré). In both cases, an experiment in the fusion of history and philosophy takes place, but in the former case it is epistemology to set the questions, while in the second it is history.

I have analysed elsewhere the development and ultimately the failure of this fascinating and ambitious ideal within the institutional spaces of 1920s, 1930s and 1940s Paris, and I have shown how the work of Gaston Bachelard represented the renunciation of total history and of the philological analysis of sources in favour of a more philosophical approach, mainly concerned with epistemological questions (Chimisso, 2001a, Ch. 5; 2001b). I have also shown how the disintegration of that theoretical ideal went hand in hand with a weakening of the links among departments and groups dedicated to the study of science. The Institut d'Histoire des Sciences et Techniques in the 1930s represented a point of contact between different approaches to the study of science. Abel Rey was also a personal friend of Henri Berr and Lucien Febvre. Together with the ideal of merging the different approaches to history with philosophy, apparently the role of the Institut as point of contact between research programmes and scholars failed as well.

Here I shall focus on texts rather than the institutional and intellectual networks, but my reading will be guided by the historiographical questions posed by the project of a synthesis between history and philosophy developed in the Parisian academia in the first half of the twentieth century. I shall assess Canguilhem's work as one of the solutions that the tension between history and philosophy produced. My specific aim, however, is that of assessing the relation of history and philosophy in the work of Canguilhem, and in particular his theory and practice of normative history.

But can we still use the heading 'history of science' for Bachelard's and Canguilhem's work?

<sup>4</sup> At the Centre de Synthèse, Rey directed the Unit of natural sciences and later the Unit of general synthesis.

<sup>5</sup> Cavaillès, mathematician and philosopher, founded the journal *Dialectica* together with Bachelard. During the war, he played a prominent role in the resistance; he was captured and killed by the Nazis in 1944. Canguilhem's commemorative speeches in honour of Jean Cavaillès are published in a small volume (Canguilhem, 1976), republished by Allia in 1996.



ilhem's work? Indeed, not only the objects of their works, but also their scope, may raise doubts about their disciplinary classification. Are they really histories? Foucault himself regarded the work of Bachelard and Canguilhem, together with that of Cavaillès, as at the basis of the *philosophie du savoir*, opposed to the other great philosophical current of post-war France, that of philosophy of existence (Foucault, 1985). In Bachelard's and Canguilhem's epistemological histories, what is more important, epistemology or history? And can the two be disentangled?

### 3. The laboratory and the tribunal: some background to the normative turn in history of science

In *La formation de l'esprit scientifique* (Bachelard, 1993 [1938]), Gaston Bachelard investigated the general conditions of the development of science. He was certainly after a certain kind of totality, in that he believed that the 'mentality' that he found behind a scientific tradition in fact informed the whole cultural production of a certain epoch. However, he did not pursue the totality that the members of the Centre de synthèse were after, or that Metzger described in the quotation above. For Metzger, everything was relevant, because the 'mentality' of a certain epoch is to be studied for itself. In other words, for Metzger the challenge was to understand the way of thinking behind past texts she studied, and the analysis of all that remains from that period is useful to meet it.<sup>6</sup>

Bachelard's history, by contrast, was a judged history; in his words:

The history of the sciences is essentially a judged history . . . The historian of the sciences, in order to judge the past properly, needs to know the present. (Bachelard, 1972 [1951], pp. 141, 142)

Bachelard intended to sort the scientific from the non-scientific and to explain the mechanisms of both. He dedicated keen attention to the imagination and emotions. These, for Bachelard, are only to be expressed in reveries, poetry and art: employed as organs of the inquiry into nature, they prevent objectivity, and produce obstacles to scientific knowledge. Some of the main obstacles that Bachelard analysed in *La formation de l'esprit scientifique* are: a sexualised view of nature, an attraction for small and precious objects, the instinct to possess these objects and the consequent desire for them to be 'real', and a disposition for generalisation and for attributing a soul, or at least life, to any object or substance. Bachelard thought that the non-rational part of the human mind did not change, and therefore the obstacles to scientific knowledge tended to turn up in essentially the same form. It was then not necessary for him to reconstruct a historical milieu in order to understand a past theory or text. He could find the same attitudes in an eighteenth-century alchemy text as

<sup>6</sup> Metzger theorized total history without really practising it; for most of time she focused on 'scientific' texts.



in a nineteenth-century poem, as for him these were constant features of the human mind (Bachelard, 1993 [1938]).

For Bachelard, the history of science thus began when knowledge had been disentangled from imagination, feelings and personal ends. Unlike the imaginative approach to nature, rational and objective knowledge was in his view able to evolve, and had a history. Bachelard's kind of history does not require a philological attention to documents, or a careful reconstruction of milieus or institutions. Actually, he did not even aim to write a history, for his objective was rather that of examining the nature of scientific inquiry. By using modern science as a norm, he was able to isolate the profound differences, or ruptures, which separated science from previous inquiry into nature, or as he called it, pre-science. Bachelard's aim was then epistemological rather than historical.

Georges Canguilhem followed Bachelard in this normative view of history of science and in the epistemological end of historical research. Canguilhem certainly inherited Bachelard's way of conceiving of the relationship between history and philosophy. He also inherited his disregard for historical research as conceived by historians who did not intend to 'judge' history of science. This may be the reason why some of them did not welcome Canguilhem's appointment to the Institut's directorship in 1955. One was René Taton, at the time research director at the CNRS. In a letter to Georges Sarton, he commented that the new appointment hardly left any hope that the Institut d'Histoire des Sciences et Techniques would 'awaken and play an active role' (Taton, 1956). At that time, the Institut's journal *Thalès*, which had been an important organ of innovation in history of science, was not even being published. Its publications were resumed in 1958, after a five-year gap. The activities of the Institut and the much reduced breadth of its output show that the directors Bachelard and Canguilhem did not pursue the goal of broad participation in a wide network which had characterized the early years of the Institut. The many institutional links of the Institut under Abel Rey's directorship (1932–1940) reflected the inclusiveness of the approaches Rey and his associates took to the history of science.

The interest of French philosophers in intellectual history dated back to the mid nineteenth century. As Martial Gueroult has pointed out, Antoine Cournot (1801–1877) already conceived of philosophy as knowledge of the mind achieved through the study of 'the efforts made, in the course of history, by the intelligence in order to constitute a science' (Gueroult, 1988, p. 877). Cournot's conception of philosophy would be developed and consolidated after him. Léon Brunschvicg, Bachelard's supervisor on one of his doctoral theses and his recognised mentor, reconstructed his own practice of philosophy as a reflection on the limits and capabilities of the mind through the study of the history of science (Gueroult, 1988, p. 877). This role of history also explains the prominence of history of philosophy in the philosophy curriculum at the Sorbonne and in general its prestige as a philosophical subject.<sup>7</sup>

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<sup>7</sup> One of the indicators of the importance of history of philosophy is the number of chairs in history of philosophy at the Sorbonne. Professors, when they obtained a chair, could choose its name; therefore the number of chairs in a certain discipline reflects the interests of the most distinguished academics. Between its foundation (1808) and 1935, at the Faculty of Lettres of the Sorbonne 4 professors chose

An example of the philosophical role of history of philosophy is exemplified by the unsigned editorial of the first issue of *Revue d'histoire de la philosophie*, attributable to the director Emile Bréhier (Anonymous, 1927, p. 4). The author argued that history was the substitute of the experimental method in philosophy: in order to study the mind, we need to see it 'at work', and history represents the laboratory in which to carry out observations. This metaphor of intellectual history as the experimental method, or the laboratory, of philosophy was rather successful. Canguilhem himself employed it, attributing it to Edward J. Dijksterhuis (Canguilhem, 1994 [1968], p. 12; Canguilhem, 1993 [1977], pp. 12–13). Canguilhem, however, argued that the tribunal or a school<sup>8</sup> would be a better metaphor than the laboratory. This tribunal is 'an institution and a place in which judgements on the past of knowledge, or on the knowledge of the past, are passed' (Canguilhem, 1994 [1968], p. 13). A tribunal, Canguilhem goes on, needs a judge: in the history of science tribunal, the judge should be epistemology (Canguilhem, 1994 [1968], p.13).

The metaphor of the tribunal provides us with a good example of the difference between Canguilhem's conception of the relation between history and philosophy and that of previous historiography of science, at the height of the drive to combine the two without sacrificing either. In 1935 Hélène Metzger gave a talk at the Centre de synthèse historique titled 'Tribunal de l'histoire et théorie de la connaissance scientifique' (Metzger, 1935b, pp. 1–14). Metzger's tribunal is rather different from Canguilhem's. First of all, the terms of the problem are reversed: Metzger addresses the question of whether *history* can pass definitive judgements on the theory of knowledge. If history is to provide definitive answers to epistemological questions by showing the development of the human mind, Metzger argues, then the theory of scientific knowledge would no longer belong to philosophy. She declared this hypothesis untenable. For her, only 'historians who are a little naïve' can think that the mere accumulation of scientific texts can reveal 'the real development of our intelligence' without any interpretation and critique (Metzger, 1935b, p. 8). Her position here could be taken as an attack on some people in the audience. The Centre de synthèse, where she was giving her talk, had a full programme of collection of documents and creation of comprehensive dictionaries aimed at the realisation of 'total history' or 'historical synthesis', in the expression of its founder and director Henri Berr (Berr, 1911, 1921, 1925). Metzger was well aware of history as reconstruction; in this very talk she declared that 'history is nothing outside the intelligence of the historian, and physics is nothing outside the physicist's intelligence' (Metzger,

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the chair in history of philosophy, 6 in history of modern philosophy, 6 in history of ancient philosophy, 2 in philosophy and history of philosophy, and 1 history of philosophy in its relationship with the sciences, for a total of 19 chairs. In the same period, the other philosophy chairs were: 8 chairs of philosophy, 1 of philosophy and opinions of philosophers, 2 of aesthetics, and 2 of psychology and philosophy, for a total of 13 chairs, to which one could add 3 of science of education, one of which was renamed by Durkheim science of education and sociology. The doctoral theses in history of philosophy constituted the single largest group of philosophy dissertations in the 1930s: 161 in 1937, followed by psychology (71). (Source: Guigue, 1935, pp. 14–29.)

<sup>8</sup> The metaphor of the school had often been employed by Bachelard; see for instance Bachelard (1993) [1938], p. 252.

1935b, p.10). The reactions to Metzger's talk were rather varied: some, like Henri Berr and Alexandre Koyré, thought that she did not give history enough importance, others thought she gave it too much.<sup>9</sup>

Koyré's interventions, though brief, are of interest here. First of all, he declared that the history of science could provide philosophy with important lessons, more than science could. He defended his claim by arguing that by studying different mentalities, we can discover what is permanent in our mind. Current science for him could not provide this because, he implied without saying, it is only one possible version of our knowledge of the world, and we cannot know whether it is the definitive truth. Koyré added that Duhem and Mach were right to try to prove their positivism through history.

Metzger was equally critical of the philosophers who thought that they could dispense with history. She included the Cartesians in this category, and Auguste Comte. Comte did refer to history of science, had a theory of history in his law of three stages, and, as Metzger reminded her audience, was the first to promote a chair of history of science in France.<sup>10</sup> However, Metzger denied that history played any serious role in Comte's philosophy. Comte, who, she argued, was not inclined to 'patient and meticulous historical study', did employ historical examples, but these only proved what they had been set to prove, that is, the truth of pre-imposed models, such as the law of the three stages (Metzger, 1935b, p. 4). She concluded her talk by stating the lessons that history of science could provide: first of all, to 'cure philosophy' of the 'strange mania to want to pose *a priori* or *a posteriori* definitive concepts', which for her only satisfied a psychological need of certainty (Metzger, 1935b, p. 13). Moreover, she argued that the awareness of the possible responses to similar experiences, and of the different consequences drawn from similar hypotheses, should cure one from dogmatism as well as from 'pointless scepticism'. Although Metzger declared that the observation of the plasticity of the mind was very important for scientific research itself, it is clear that for her the lessons to be drawn from history are philosophical ones, about the variety of ways in which our knowledge of the world can be organized and expressed.

Notwithstanding her firm conviction that history of science could teach us a few lessons about the mind, Metzger refused to judge the truth content of past theories by using an external norm. Indeed, her efforts went towards understanding the 'mentalities' behind doctrines which may at first seem absurd. For instance, it may seem peculiar that the philosophers of metals never doubted the possibility of turning lead into gold, despite their frustrating lack of success. However, Metzger argued that when one considered their world-view, they appeared to be much less irrational than at first sight. They saw all things in a relationship of analogy. For them lead was to gold what an infant was to a man: just a less developed, or less perfect version of

<sup>9</sup> The minutes of discussion which followed Metzger's paper are published in *Archeion*. See *Fondation 'Pour la science', Centre International de Synthèse, Section d'histoire des sciences* (1935), pp. 81–84. For a comment on this session see Redondi (1997).

<sup>10</sup> For a history of the chair of general history of the sciences see Paul (1976); and for an account of the fortunes of history of science in France in the first half of the twentieth century, see Chimisso (2001a), Ch. 5.

the same thing. The actual transformation of one into the other was something simply not yet achieved, but perfectly consistent with the order of things (Metzger, 1969 [1923], pp. 108 ff.). Thus, Metzger aimed at finding the internal norm that governed a certain period of intellectual history. She called this norm the *a priori* of thought.<sup>11</sup>

Koyré's position was different from Metzger's, although not completely opposed to it, especially as we see it in his work on history of science, the publication of which commenced a few years after that seminar at the Centre de synthèse. Koyré too aimed at understanding the ways of thinking of natural philosophers. What he called 'conceptual structures' is similar to Metzger's *a priori*, and both these notions have strong connections with Lévy-Bruhl's mentality. Koyré believed that conceptual structures and world-views changed in intellectual history, and quite significantly too. The name of Koyré is indissolubly connected with that of the 'scientific revolution', for he concentrated on the rupture which divided the Greek-medieval 'closed' world from the open universe of modern cosmology (Koyré, 1958), and the quantitative approach of Galileo and Newton that broke with the qualitative approach of previous Aristotelian science (Koyré, 1939, 1965). Nicholas Jardine has argued that Koyré held that the ontologies resulting from a period's conceptual structures were real entities, rather than mere useful historical categories (Jardine, 2001, p. 16). To rephrase Jardine's point into Canguilhem's language (and theory), Koyré assumes a series of norms, e.g. Aristotelianism, Platonism and the Scientific revolution, that he employs to judge texts and ideas, and that do not depend on other norms. Koyré, however, sometimes slipped in an unclarified 'metanorm' which allowed him to judge those very ontologies. For instance, when Koyré judges Giordano Bruno 'a very poor scientist' (Koyré, 1958, p. 54), or when he declared that 'Aristotle's physics is false, it is irremediably superseded' (Koyré, 1978 [1939], p. 4), he was not evaluating these philosophies by the standards of their times, but by an external norm. In these cases, Koyré did judge past theories, as much as the historian Metzger avoided giving 'good marks to some scientists and bad marks to others' (Metzger, 1935b, p.14).

In the eyes of many historians, Koyré remained a philosopher, and only a philosopher. Jacques Le Goff has remarked, while recognizing his 'attentive reading of texts', that the 'great Koyré' 'could not push his investigation further than the analysis of concepts and systems'. He concluded that:

... he always remained a philosopher, a type of scholar with whom, at least in France, historians have always had difficulty in establishing a dialogue, due to their distrust for the philosophy of history. (Le Goff, 1983, p. 412)

In Le Goff's view, Koyré's 'attentive' reading of texts did not automatically make Koyré a historian in the historians' eyes. Although Le Goff did not go into details, it is not too arduous to guess some of the reasons for his criticism. For a historian of mentalities like himself, the challenge was to examine the links between ideas and social realities, while Koyré dispensed with the latter. To Le Goff, Koyré's

<sup>11</sup> See, for instance, Metzger (1936).

analysis of ‘concepts and systems’ must have appeared the ultimate objective of his research, while the historians’ objective should have been that of analysing the historical formation of those concepts and systems, and their possibility of existence and development in a given time and society. Moreover, although Koyré’s ‘conceptual structure’ has close links with the concept of ‘mentality’ and Febvre’s ‘*outillage mental*’, in fact it applies ‘particularly’ to thought ‘in its highest forms’ (Koyré, 1966 [1951], p. 1). In reality, Koyré only analysed high culture, and the links that he established were between philosophy, ‘science’ and religion, the latter conceived as theology rather than popular expressions of beliefs. By contrast, Febvre’s *outillage mental* was aimed precisely at the overcoming of the idea that high culture had a life independent of the broader culture, in the ethnological sense of the word.

The practice of careful reading is by no means foreign to some philosophical traditions, and Koyré’s education in Germany makes his interest in it quite unsurprising. In the context of French history and philosophy of science, however, his philological attention to texts, while in line with an existing historiographical tradition, marks an important difference between himself and Bachelard. In the split which I argued occurred in the programme of combining history and philosophy of science, one of the activities that philosophers such as Bachelard and Canguilhem dropped was precisely the philological aspect of their research. However philosophical Koyré’s interests were, his main aim was not that of explaining the working of the human mind, but more modestly the intellectual changes in the scientific revolution, or Galileo’s metaphysical assumptions.

However, general historians did not see much of a difference between Bachelard and Koyré; if anything, Bachelard was probably seen as more of a philosopher and less of a historian. As evidence of this lack of communication stands the only review of Bachelard’s books to have appeared in the *Annales*. The mouthpiece of the historians of mentalities did not as a rule host the work of the historians and philosophers of science. Roger Chartier has argued that this lack of interest bore negative consequences for French historiography. For him French historians’ confidence in quantitative methods and their faithfulness to their own definition of history of mentalities prevented them from recognizing ‘the importance of the epistemological model offered by the work of Gaston Bachelard, Alexandre Koyré and Georges Canguilhem to the procedures of intellectual history’ (Chartier, 1988, p. 35).

This lack of communication may come as a surprise especially because the contacts between Febvre and the historians of science knew no practical obstacles; indeed Febvre was on the management committee of the Institut d’Histoire des Sciences et Techniques (*Thalès*, 1 (1934), p. xi) and for two years (1943–1945) he was in the same department (Religious Studies) of the Ecole Pratique des Hautes Etudes as Koyré. He was also an important member of the Centre de synthèse, but rarely ventured to the Unit for history of science.<sup>12</sup> The weakness of the relations

<sup>12</sup> Febvre did attend Metzger’s talk on ‘Les différents aspects de la même époque d’une civilisation (lettres, sciences, arts) peuvent-ils être considérés comme autant de projections variés d’un même état d’esprit? Ou au contraire leurs modifications diverses ont-elles agi individuellement sur l’évolution de cette civilisation en général?’ (Fondation ‘Pour la science’, Centre International de synthèse, Section d’histoire des sciences, 1930).

between Febvre and the ‘historians of science’ should, however, be qualified, not least chronologically. Febvre did collaborate with Abel Rey. They edited the *Revue de synthèse historique* together, and Febvre, as general editor of the *Encyclopédie française*, entrusted the first volume, on the ‘*outillage mental*’, to Abel Rey (together with Antoine Meillet and Paul Monteil). In *Le problème de l’incroyance au XVI<sup>e</sup> siècle* (Febvre, 1942), Febvre quoted Rey on various matters, including Greek science (pp. 393, 403, 432, 437), Duhem on physics (p. 384), Lynn Thorndike (pp. 382, 396), and other historians and philosophers of science.

In a review of works in history of philosophy, Febvre lauded ‘Abel Rey’s collaborators’ for their work of editing and translating philosophical texts, for they paid attention not only to ‘pure ideas’, or their ‘logical filiation’, but also to their historical emergence and their relationship with the wider context (Febvre, 1992 [1938], p. 283). However, it is quite clear that the series he was reviewing was for him the exception rather than the rule as far as the methodology of historians of philosophy was concerned. He thus excluded, I suspect, most historians of philosophy and of science from the community of historians:

Of all workers who cling to the generic title of historian, with or without a qualifying adjective, there are none who fail to justify some part of it in our eyes save, all too often, those who, applying themselves to rethinking for their own purposes systems that are sometimes several centuries old, with not the slightest effort to show their connections with other manifestations of the epoch which saw their rise, end up doing precisely the opposite of what historical methods demands. Then, faced with these concepts engendered by disembodied minds—which take on a life of their own outside time and space—they forge strange chains the links of which are both unreal and closed.<sup>13</sup>

Febvre’s criticism seems rather unfair if applied to all of his contemporary historians of philosophy, and to those philosophers who had turned their attention to the history of science. It is undeniable, though, that Febvre had little patience for the historians of science’s theoretical discussions, as showed by his interventions at Metzger’s talk. *A fortiori* it is difficult to imagine a collaboration between him and Bachelard or Canguilhem, that is to say, after the ‘normative turn’ that transformed history of science into historical epistemology. In the range of orientations of the ‘general history of science’, Febvre would have been interested only in the more historical and philological, whereas the more philosophical and epistemological ones were going to succeed. In Le Goff’s judgement, Bachelard’s work has had the same reception, or lack of it, that Koyré’s had; he points out that Bachelard, despite his ‘desire’ to write history, has appeared, ‘rightly or wrongly’, to historians as a philosopher rather than a historian (Le Goff, 1983, p. 412).

From the 1940s onwards, normative history of science opened up a new style of research, significantly contributing, in the opinion of Foucault and many others, to

<sup>13</sup> Febvre (1992) [1938], p. 278, quoted in modified English translation from Chartier (1988), pp. 22–23.



the most important philosophical debates of post-war France. I shall now investigate the differences between Bachelard's and Canguilhem's approaches in order to shed more light on Canguilhem's conception of norm and on how he applied it to history.

#### 4. Bachelard's and Canguilhem's normative history

Canguilhem advocated the normative approach to history of science in several articles and talks. When he wrote his theoretical remarks on history of science and epistemology (Canguilhem, 1993 [1977], 1963, 1994 [1968]), the normative turn in my view had been fully realized. The epistemological approach had disentangled itself from the ideal of total history and had chosen normativity over a relativistic and philological approach. The goal of combining the historical craft with philosophical aims had been abandoned, and the tension between these two poles of history of science had been resolved. It had been resolved thanks to the success of the epistemological approach in the hands of Gaston Bachelard, closely followed by Canguilhem. In his theoretical essays, Canguilhem indeed relied on the ideas and authority of his predecessor in the Sorbonne chair of history and philosophy of the sciences. Indeed, Canguilhem made important reflections on this type of history in articles on Bachelard. Bachelard's and his own method and authority were too well established to call for a polemical advocacy. Rather, at that point, that is, in the late 1960s and 1970s, Canguilhem was able to show a coherent approach which had already evolved in the hands of successive scholars, first of all Michel Foucault.

In the Introduction to *Idéologie et rationalité*, titled 'Rôle de l'épistémologie dans l'historiographie scientifique contemporaine', Canguilhem argues that, without a normative point of view, the historian of science cannot construct any history. Without a norm of her own, the historian would consider 'botanical' what was considered to be botanical in the past. Yet, eighteenth-century botany, he argues, does not necessarily constitute the past of modern botany. Indeed, Canguilhem claims that 'taken in its absolute sense, the "past of a science" is a vulgar concept' (Canguilhem, 1993 [1977], p. 13).

In his view only epistemology can solve this problem:

the history of science is entitled to expect from epistemology a set of ethical criteria, by which I mean a set of criteria for judging which moves within the vast expanse of the past are legitimate and which are not.<sup>14</sup>

What does Canguilhem mean by 'legitimate moves'? In order to understand this, I think it is worth looking at *La formation du concept de réflexe aux XVII<sup>e</sup> et XVIII<sup>e</sup>*

<sup>14</sup> Canguilhem (1993) [1977], p. 14; in English translation, Canguilhem (1988b), p. 4. I quote here from the English translation of *Idéologie et rationalité* for its clarity. However, this translation is not close to the French original: 'On peut penser que ce que l'histoire des sciences est en droit d'attendre de l'épistémologie, c'est une déontologie de la liberté de déplacement régressif sur le plan imaginaire du passé intégral'.



*siècles* (Canguilhem, 1955). In this book, Canguilhem aimed to disprove what he called two prejudices. The first is that a concept can only emerge in the context of a theory, or ‘at least within a heuristic inspiration’ which is homogeneous with the theory that will later explain the observed facts (Canguilhem, 1955, p. 3). In other words, this ‘prejudice’ denies that a concept employed in a current scientific theory could have emerged within a theory which is now deemed non-scientific, or which is incompatible with the current theory. The second, more specific, prejudice is that in biology only mechanistic theories have led to positive results and fruitful applications (Canguilhem, 1955, p. 3). For Canguilhem, these two prejudices have distorted the history of the concept of the reflex. He therefore set out not only to reconstruct a more truthful history of this concept, but also to use his version of facts as evidence against those two assumptions. The first prejudice Canguilhem eliminated was the claim that Descartes provided the first coherent formulation of the concept of the reflex. He argued that this could not be the case because, in Descartes’ physiology, the flux of ‘spirits’ in involuntary bodily movements is always from the brain towards the periphery, and never in the opposite direction. Rather, for Canguilhem, the first to propose the concept of the reflex was in fact Thomas Willis (1621–1675), professor of natural history at Oxford, and of medicine in London. Willis used this concept within a rather imaginative theory in which life is interpreted as light. The metaphor of light brought about his use of the optical laws of reflection in the interpretation of biological phenomena. The concept of reflex then came as a ‘natural’ consequence of Willis’ general theory. Canguilhem argued that Willis had been forgotten by historians because his theory had been doomed to be judged untenable. Willis’ reputation was the victim of the prejudice according to which the truthfulness of concepts depends on the truthfulness of the theories of which they are part. Canguilhem used this historical example to prove his theory that, in fact, concepts are independent, in their validity, of the theory in which they are embedded.

In *La formation du concept de réflexe*, Canguilhem blocked the ‘move’ between Descartes’ notion of ‘reflex movement’ and the modern concept of reflex, while allowing the one connecting the latter with Willis’ concept. Canguilhem’s assumed starting point was the current concept of reflex: he employed this to assess whether past concepts are related to it. His answer was negative in the case of Descartes, and positive in the case of Willis. In a Bachelardian manner, Canguilhem isolated the epistemological obstacle that prevented Descartes from having a concept of reflex, that is, Descartes’ general physiology. Canguilhem’s argument against the continuity of the modern concept of reflex with Descartes’ was not based on historical evidence, but on the analysis of the internal consistency of the various doctrines. He defended his approach when he wrote that ‘in matters of the history of the sciences the rights of logic should not be replaced by the rights of the logic of history’ (Canguilhem, 1955). The ‘logic of history’ may suggest that the modern concept of reflex had its origins in a mechanistic theory. However, Canguilhem aimed to show that the modern concept of reflex would have been inconsistent with Descartes’ mechanistic physiology: the internal logic of Descartes’ doctrine disproves, in his view, what the ‘logic of history’ suggests. What Canguilhem was interested in is the evaluation of past concepts on the basis of present ones, rather

than a study of the past in its own terms. His method should not be confused with a recurrent and anachronistic historiography, for it is indeed the opposite. Although he did read past texts with modern science in mind, he did not anachronistically force them to fit into it. He thought that his analysis of past concepts would serve to indicate which links could be established. His method was most effective in its negative part, when it allowed him to establish that certain continuities were delusions created by recurrent reading of past texts.

One of the differences between Bachelard and Canguilhem that immediately strikes the reader is that the former emphasises discontinuities, while the latter establishes both continuities and discontinuities, as for instance in *La formation du concept de réflexe*. Françoise Dagognet has argued that Canguilhem regarded discontinuities as partial and complex, as exemplified in his criticism of Alexandre Koyré's interpretation of the scientific revolution (Dagognet, 1997, p. 162). Canguilhem took exception to Koyré's claim that Galileo rejected all forms of Aristotelianism in favour of an Archimedean and Platonic world-view. Canguilhem agreed with Ludovico Geymonat that Galileo kept important aspects of the Aristotelian tradition (Canguilhem, 1993 [1977], p. 25).

It is interesting that Canguilhem criticised Koyré rather than Bachelard for exaggerating epistemological ruptures. Bachelard was not only Canguilhem's mentor and predecessor in the Sorbonne chair; he was the scholar who had given epistemological history its crucial place in the French intellectual landscape. In the division of camps following the split (which had been complex, slow and contradictory—not a sudden epistemological rupture) of French history of science, Koyré occupied a slightly ambiguous position. His links with the Sorbonne, which had been significant before the second world war, weakened considerably after the war. From a methodological point of view, Koyré did not give up a thorough attention to texts and his aim was to understand an author or period rather than reaching general epistemological or historiographical conclusions. On the other hand, Koyré's work does have a strong and explicit philosophical framework, and proposes a grand narrative that emphasises epistemological ruptures. For the latter reason, Canguilhem sometimes mentioned Koyré as close to Bachelard in his conception of history of science, even though he then stressed their differences (see, for instance, Canguilhem, 1994 [1968], pp. 13–14).

The link between Bachelard and Canguilhem has been seen as so strong that this obvious difference in their views of historical discontinuities has demanded an explanation. Dagognet has argued that their divergence originates in the sciences that they respectively studied. Bachelard analysed mature sciences, especially physics and chemistry, while Canguilhem devoted himself to the history of the life sciences. The latter, Dagognet's argument goes, were still in their infancy—that is, in a moment of slow evolution, when continuities are stronger (Dagognet, 1997, p. 162). In this observation, Dagognet follows Canguilhem himself who in turn reported Bachelard's view (Canguilhem, 1994 [1968], p. 14).

Although Dagognet points to an important feature of Canguilhem's work, this does not constitute, in my view, a full explanation of his attention to continuities. I believe that the implications of his difference from Bachelard are much more

important than this simple distinction would suggest. First of all, it is worth noting that it is not just the sciences under study that differ, the focus of their works does too. This is quite evident in the two *Formations*: Canguilhem's *La formation du concept de réflexe* and Bachelard's *La formation de l'esprit scientifique*. The breadth of scope could not be more at variance: Canguilhem analyses a specific concept within physiology, Bachelard nothing less than the scientific mind. Moreover, Canguilhem affirms the independence of concepts from the theory in which they are inscribed, so that Willis' concept of reflex can be seen as the seed of the modern concept, even if his physiological theory is completely at odds with modern science. In other words, Canguilhem establishes continuities across incompatible theories, indeed across different world-views.

By contrast, in his *Formation*, Bachelard aims to show that there is a break between pre-science and science. Most of the examples employed in *La formation de l'esprit scientifique* are drawn from the eighteenth century, which Bachelard characterises as part of the 'pre-scientific period', which for him stretches from antiquity to the end the eighteenth century. He argues that no science was possible in that period. For Bachelard scientific knowledge emerges by contradicting previous conceptions. For instance, while the study of metals was dominated by sexual imagery, no chemistry could develop; alchemy flourished instead, with its interpretation of metals as gendered and of mixtures of substances as copulation. Bachelard denies that a scientific concept, or the root of a scientific concept, could emerge not only within a non-scientific theory, but in general within a non-scientific mentality. In what he calls the pre-scientific period the whole approach to nature could not be conducive of objectivity, as it was dominated by an imaginative world-view, guided by subjective needs and desires. Moreover, scientific discourse for Bachelard is only produced socially, through discussion, rather than by isolated individuals. The social production of scientific discourse requires rational and objective relationships between individuals that for Bachelard did not take place in the pre-scientific period. Private relationships, based on personal authority, cannot generate objective knowledge, and, before the nineteenth century, he can observe neither the psychological nor the social conditions for science. He warns that, though pre-scientific concepts may seem similar to scientific ones, actually they are at odds with anything scientific, since they are the results of subjective, imaginative and a-rational approaches. In his view, the modern sciences did not inherit any positive contents from natural philosophy of any kind. Rather, they developed by opposition to previous knowledge. For Bachelard, without the dialectical overcoming of epistemological obstacles, no science is possible, and therefore no scientific theory and no scientific concept. It is safe to conclude that the continuity between Willis' concept of reflex and the modern one established by Canguilhem is in contradiction with Bachelard's theory of the development of science.

Canguilhem's view of discontinuity is certainly more complex, more partial and fragmented than Bachelard's, and does not necessarily take the shape of an overall change of mentality and social relationship as does Bachelard's. Is the difference in their views a consequence of the fact that the former studied 'mature' sciences while the latter studied 'young' sciences? Canguilhem establishes continuities, such as that

of the concept of reflex, that go back four centuries—much longer than any continuity accorded by Bachelard to physics and chemistry. For Bachelard ‘no eighteenth-century observation has originated a nineteenth-century technique’ (Bachelard, 1993 [1938], p. 71). Paradoxically, Canguilhem’s physiology, or at least some of its concepts, is much older than Bachelard’s physics. Although this difference in their object of study is very important, I believe that it is insufficient to resolve their divergence on the issue of the epistemological rupture.

## 5. Historical epistemology and the living being

In *La formation du concept de réflexe*, the norm is provided by current science. There is no discussion, though, of how science produces normative concepts, and whether their origins lie inside or outside science. The question of how a norm emerges remains unanswered. However, when *La formation* was published, Canguilhem had already written a history of the concept of norm in his *Le normale et le pathologique*. This book well exemplifies the tenets of historical epistemology: its main goal is to discuss and clarify the concepts under study, their origin and their applications; and the way of investigating them is historical. The book is divided into two parts, aimed to answer the questions posed in their titles: ‘Is the pathological state merely a quantitative modification of the normal state?’ and ‘Do sciences of the normal and pathological exist?’. Canguilhem describes the first part as research of historical sources, and the second as the critical presentation of his own doctrine. Indeed, the chapters of the first part focus more specifically on past doctrines, from references to the Egyptian and the Hippocratic tradition to in-depth discussions of Auguste Comte’s, Claude Bernard’s and René Leriche’s ideas. The material of the second part is arranged according to issues: first an examination of the concepts of normal, anomaly and disease, and of normal and experimental, then discussions of the relation between the norm and average, between disease, cure and health, and between physiology and pathology.

However, the method that Canguilhem follows in the two parts is not radically different: in both cases it is a critical dialogue with other doctrines. The first part is not historical in the sense in which most historians would understand history: there is no attempt to build a historical narrative, or to reconstruct a historical milieu. What Canguilhem’s approach does have in common with that of Metzger, Koyré and the historians of mentalities is that it links conceptions of illness and health to more general metaphysical assumptions. A good example of this method is Canguilhem’s treatment of the change from a qualitative to a quantitative conception of disease.

Canguilhem explains that the quantitative conception of disease, the champion of which is Claude Bernard (1813–1878), is that according to which the pathological state is a quantitative variation of the normal state. For instance, the presence of sugar in the bloodstream is a normal physiological phenomenon. In the diabetic, the quantity of sugar is higher: the pathology consists in a quantifiable variation of a normal state, rather than in a qualitative difference. This conception differs from the

Hippocratic view of disease, whereby illness is seen as a state of disequilibrium qualitatively distinct from the harmonic state of health. It is also at odds with an ontological view of the disease, exemplified in modern times by the microbiological theories of contagious diseases (Canguilhem, 1999 [1966], p. 12).

The emergence of the nineteenth-century quantitative conception of disease for Canguilhem rests on a series of philosophical premises which emerged from the Renaissance onwards. The more general one concerns the relationship between man and nature. In the Hippocratic view, human beings were part of nature, and their internal equilibrium was the same as that outside them (Canguilhem, 1999 [1966], p. 12). With Francis Bacon, the unity of human beings and nature was broken; since then, the dominant view was that human beings could and must intervene in nature in order to achieve their goals (Canguilhem, 1999 [1966], p. 13). Intervention in the course of nature was aimed at restoring a desirable state of affairs. In the case of medicine, this was health, hence the need to have a good knowledge of the healthy state. The goal of intervention made physiology become the basis of pathology, which eventually became a branch of the former. The identification of physiology and pathology made the qualitative view of disease untenable, and lead, for Canguilhem, to the quantitative view (Canguilhem, 1999 [1966], pp. 13–14).

In Canguilhem's view, the quantitative conception of disease not only needed a Baconian view of the relationship between man and nature, but also the dissociation of science and religion, and the emergence of nineteenth-century 'rationalistic optimism'. This optimism, Canguilhem explains, denied the reality of evil and the associated 'Manichean' view that 'Health and Disease fought over man the way Good and Evil fought over the World' (Canguilhem, 1999 [1966], p. 61). Claude Bernard's quantitative and optimistic view also rested on the positivism of its time, exemplified in particular by Comte, according to which a technology is the application of a science, as summarized in the motto: 'to know in order to act'.<sup>15</sup> Following this general assumption, physiology explained pathology which in turn provided the basis of a therapeutics. This implied the monistic idea of the qualitative indifferetiation of illness and health.

Following a methodology similar to that of Metzger and Koyré, Canguilhem links historical concepts of health, disease and normality to broader world-views and philosophical systems, but, unlike them, his scope is pre-eminently epistemological and normative. Canguilhem analyses past doctrines not for their own sake, but in order to reach conclusions about the concepts of the normal and the pathological. In his own words, the historical part of his book served to establish the 'narrowness and inadequacy of the principle of pathology . . . according to which the morbid state in the living being is only a simple quantitative variation of the physiological phenomena which define the normal state of the corresponding function'.<sup>16</sup>

In order to understand Canguilhem's own concept of norm, his distinction between norm and average is particularly illuminating. He discards the theories which equate

<sup>15</sup> Canguilhem (1999) [1966], p. 58; in English translation, Canguilhem (1989), p. 99.

<sup>16</sup> Canguilhem (1999) [1966], p. 155; in English translation, Canguilhem (1989), p. 277.

norm and average. In his view, the norm cannot be calculated from a collection of data, because it represents a desired state of affairs rather than an average. Canguilhem insists that the norm refers the real to values, and therefore it cannot be reduced to an objective concept that we can determine scientifically, either in physiology or elsewhere. Human beings are normative, which means that they establish norms, and discard old norms in favour of new ones (Canguilhem, 1999 [1966], p. 106). In other words, they are not merely the expression of norms, or the embodiment of given biological realities (as in ‘the normal man’), but rather they create their own norms. Indeed, if Canguilhem has a concept of human nature, it seems to lie precisely in the human ability to adapt to new situations by establishing new norms.

To illustrate what Canguilhem means, one can say that an individual has a concept of good health and fitness and she judges her own state by this normative concept. This concept of good health does not correspond to a normal state of affairs, for most people are not continuously, if ever, in this state of good health and fitness. However, as a rule human beings do not aim at being average, for example trying to catch an average number of colds and flus, but generally, although not necessarily, they try to get as close as possible to the normative concept of good health. This normative concept of good health varies depending on a variety of factors. An obvious one is age: a seventy-year-old would not have the same norm as a twenty-year-old. Canguilhem especially emphasises that what counts as normal depends on the creative way in which groups of human beings have adapted to their environment and life circumstances. For instance, he believes that ethics and religion play an important role in human beings’ determination of ‘normality’. He cites the example of yogis, who are able to alter their vegetative functions in a way that would be dangerously pathological for other people. In the yogis’ case, there is nothing pathological: they have simply instituted different norms (Canguilhem, 1999 [1966], pp. 106–107).

Canguilhem even claims that illness can be seen as a type of normality. This apparently paradoxical claim is explained by bearing in mind that for him illness also refers to norms, rather than being just a neutral disorder. A diabetic’s life is regulated by norms created by her new relationship with her environment. What really changes in the pathological state is that the patient is dominated by these norms and loses the ability to adapt to new situations (Canguilhem, 1999 [1966], p. 120). The process of healing consists for Canguilhem in imparting new norms on life, norms which are clearly superior to the old ones (Canguilhem, 1999 [1966], p. 156), for patients, once recovered, are able to respond more positively and creatively to their environment.

This creative response to the environment is particularly important because the human environment is unstable and requires different responses at the different times. In his words: ‘health is a margin of tolerance for the inconstancies of the environment’. Canguilhem’s defence of his own view that the environment is inconstant is very interesting because it is centred on the obvious, but often neglected, fact that the scientific image of the world (the world of laws and regularities) is not the world in which human beings live. His observation deserves a long quotation:



Health is a margin of tolerance for the inconstancies of the environment. But isn't it absurd to speak of the inconstancy of the environment? This is true enough of the human social environment where institutions are fundamentally precarious, conventions revocable, and fashions as fleeting as lightning. But isn't the cosmic environment, the animal environment in general a system of mechanical, physical and chemical constants, made of invariants? Certainly this environment, which science defines, is made of laws but these laws are theoretical abstractions. The living creature does not live among laws but among creatures and events which vary these laws. What holds up the bird is the branch and not the laws of elasticity. If we reduce the branch to the laws of elasticity, we must no longer speak of a bird, but of colloidal solutions. At such a level of analytical abstraction, it is no longer a question of environment for a living being, nor of health nor of disease. Similarly, what the fox eats is the hen's egg and not the chemistry of albuminoids or the laws of embryology. Because the qualified living being lives in a world of qualified objects, he lives in a world of possible accidents. Nothing happens by chance, everything happens in the form of events. Here is how the environment is inconstant. Its inconstancy is simply its becoming, its history.<sup>17</sup>

For Canguilhem, normality is a qualitative concept rather than a quantitative one, and so is abnormality. Just like the normal, what counts as abnormal, or pathological, for Canguilhem refers to values rather than facts. Pathology involves pathos, suffering and disempowerment, whereas anomaly does not. The difference between abnormality and anomaly then lies on the subjective experience of the sick person, and on the values she attaches to her state. Similarly, Canguilhem thinks that the human body's normal state that medicine aims to restore is normal by virtue of the fact that the sick person deems it to be normal (Canguilhem, 1999 [1966], p. 77). Subjective here does not merely mean individual. Indeed, Canguilhem rightly thinks that values are generated socially. Notwithstanding his discussion of the difference between biological and social norms,<sup>18</sup> he believes that biology and sociology are inextricable. This is clear in his evaluation of biological facts in social and cultural terms, which is strictly connected with his conviction that a human trait is normal not because it is statistically frequent, but is frequent because it is normal. In other words, if, in a certain form of life, something is considered to be the norm, it will occur more frequently. For instance, life expectancy depends on social norms of hygiene, work conditions, attention to fatigue and disease. Canguilhem concludes that:

the techniques of collective hygiene which tend to prolong human life, or the habits of negligence which result in shortening it, depending on the value attached

<sup>17</sup> Canguilhem (1999) [1966], pp. 130–131; in English translation, Canguilhem (1989), pp. 197–198.

<sup>18</sup> See in particular the first of the three 'Nouvelle réflexions' appended to *Le normal et la pathologique*, written twenty years after the first edition of the book: 'Du social au vital' (Canguilhem, 1999, pp. 176–191).



to life in a given society, are in the end a value judgement expressed in the abstract number which is the average human life span.<sup>19</sup>

Canguilhem also maintains that social norms may have their origin in biological norms. For instance, he argues that there would be no medicine if there were no biological norm that makes organisms preserve themselves. He does not attempt an impossible and ultimately pointless distinction of social norms and biological ones.<sup>20</sup> Canguilhem believes that life poses values on its own environment and also on the organism itself. It is then ‘life’ that generates the norm. What Canguilhem means by ‘life’ is an undivided whole including biology as well as sociality, rationality as well as emotions. The normal is a value and not a fact, and this value is established by the ‘organism’, or the human being in his or her entirety: as a biological, social, rational and emotional being.

*Le normale et le pathologique* is a normative book about the norm and normativity. Is the normativity Canguilhem discusses in *Le normal et la pathologique* the same normativity that he employs in his practice of normative history? Is his historical epistemology based on the concept of norm exposed and espoused in his writing on medicine? Once again, a comparison with Bachelard will shed light on these issues.

## 6. Medical norms and historical norms

Canguilhem defines the concept of normative in the following manner:

*Normative*, in philosophy, means every judgement which evaluates or qualifies a fact in relation to a norm, but this mode of judgement is essentially subordinate to that which establishes norms. Normative, in the fullest sense of the word, is that which establishes norms.<sup>21</sup>

This definition naturally provokes the question of what establishes the norms. In an article on Canguilhem, Michel Fichant asks a similar question: where does the norm come from? (Fichant, 1993, p. 38). Fichant’s answer to his own question is, in brief, that for Canguilhem the norm comes from the living being’s values. The living being creates values which establish norms. Fichant contrasts Canguilhem’s position with Bachelard’s, according to which the norm is given by mathematization.

<sup>19</sup> Canguilhem (1999) [1966], p. 103; in English translation Canguilhem (1989), p. 161.

<sup>20</sup> Malcom Nicolson has argued that Canguilhem’s thesis of the continuity of biological and social norms would provide a solid basis for current sociology of scientific knowledge. He maintains that ‘not only can [*Le normal et la pathologique*] help us towards a more sophisticated account of the materialist basis of scientific knowledge; it also provides us with grounds from which to argue that the acknowledgement of such a materialist basis need not entail any weakening of the strength or scope of the sociological approach to scientific knowledge in general or of relativism or constructionism in particular’ (Nicolson, 1991, p. 361).

<sup>21</sup> Canguilhem (1999) [1966], p.77; in English translation, Canguilhem (1989), pp. 126–127.

Fichant's comparison take us back to the question of the objects of normative histories. For Bachelard current science sets the norm. He focuses on physical sciences, and therefore current physical sciences, which are quantitative, provides his norm of scientificity, and consequently of his historical epistemology. Bachelard's view of physical sciences is that they are not only obviously quantitative, but also rational, objective and based on a dialogue between practitioners. He argues that their rationality is the result of a process of purification from irrational and emotional perspectives that practitioners would instinctively bestow on them. For him their objectivity stems from the same process of rationalisation: he identifies subjectivity with imagination, desires and private goals; only by removing the latter does he think that knowledge can achieve objectivity. In his view the quantitative and social character of science are indissolubly linked; in his words: 'The isolated individual's science is qualitative. Socialized science is quantitative' (Bachelard, 1993 [1938], p. 242).

Like Bachelard, Canguilhem thinks that philosophy should follow science: at the beginning of *Le normal et la pathologique*, Canguilhem claims not only to take current science as a benchmark, but also to aim at an integration of scientific results into philosophy.

The present work is . . . an effort to integrate some of the methods and attainments of medicine into philosophical speculation . . . we want to contribute to the renewal of certain methodological concepts by adjusting their comprehension through contact with medical information.<sup>22</sup>

Canguilhem's view of the relationship between science and philosophy is here less univocal and binding than Bachelard's. Moreover, Canguilhem aims to integrate into philosophy methods and attainments of medicine and the life sciences, rather than physics and chemistry. However contingent Bachelard's and Canguilhem's respective choices of sciences may have been—depending, *inter alia*, on their biography—they engendered a philosophical difference of crucial importance. First of all, in the sciences that Canguilhem examines, the object is in a position to establish norms, that is, to participate in the creation of science itself. This is most evident in medicine and psychiatry. Indeed, the very existence of medicine depends on the patients' values:

we think that medicine exists as the art of life because the living human being himself calls certain dreaded states or behaviours pathological (hence requiring avoidance or correction) relative to the dynamic polarity of life, in the form of negative value.<sup>23</sup>

<sup>22</sup> Canguilhem (1999) [1966], p. 8; in English translation, Canguilhem (1989), p. 34.

<sup>23</sup> Canguilhem (1999) [1966], pp. 76–77; in English translation, Canguilhem (1989) p. 126.

The objects of these sciences are also subjects, and their values guide scientific development:

The sick person is a Subject, capable of expression, who recognizes himself as a subject in all that which he does not know how to designate other than with possessives: his pain and the representation that he makes of it, his angst, his hope and his dreams. (Canguilhem, 1994 [1968], p. 409)

‘Angst, hope and dreams’ are not then epistemological obstacles to be overcome, as they are for Bachelard. In medicine and psychiatry as conceived by Canguilhem, feelings and emotions contribute to creating the field of intervention. A state of affairs is pathological, and calls for medical intervention, because the patient experiences suffering. For Canguilhem, the patient’s experience is irreducible to any scientific objectivity. However, in his view, the patient’s subjectivity and conception of illness are not merely individual. Rather, they are consequences of ‘culture and history’ (Canguilhem, 1994 [1968], p. 409). He points out that modern western individuals, who live in ‘industrial and democratic’ societies do not share Pascal’s view that illness is human beings’ natural state, while health is a danger for their souls (Canguilhem, 1994 [1968], p. 409). Obviously, these different cultural views produce different conceptions of medicine and of the proper extent of its intervention.

Canguilhem agrees with Bachelard that norms are produced socially. However his meaning of ‘society’ does not overlap with Bachelard’s. When Canguilhem spoke of socially produced norms, he referred to any society, as community of people, while Bachelard intended a scientific community. In Bachelard’s ‘scientific city’ the culture is one of rationality and objectivity (Bachelard, 1986 [1949], Ch. 7). In society at large, the production of norms is far more comprehensive and complex. Canguilhem recognises that feelings, emotions and desires are also dependent on culture and history. Fear of illness, or even endurance of pain, can be quite different in different cultures, in which different values are attached to the presence or absence of illnesses and pain. By contrast, for Bachelard scientific norms are created only by scientific activity, by denying and contradicting the norms created by everyday life and experience. A possible consequence of Canguilhem’s arguments is that the establishment of norms is not confined, as for Bachelard, to realm of the experts, and is potentially more broad-based and democratic. However, this is not quite the consequence that Canguilhem drew. He called for a revision and enlargement of scientific rationality so as to consider the irreducible subjectivity of patients, as well as the limits of scientific rationality, but he never thought that non-experts should take their health into their own hands. His comments on the diffusion of ‘pre-rational’ medicine and the growing refusal of official medicine that he saw in the 1970s are revealing of his position. He thought that they were evidence of the need for a reflection on medical rationality and on its limits, but he called them an ‘amalgam of banalities’ (Canguilhem, 1994 [1968], p. 401).

Canguilhem’s conclusions are not, in my view, simply a consequence of his studying life sciences, but rather of his conception of what life sciences should be. He advocated a type of medicine and psychiatry in which patients’ feelings and emotions

and their subjectivities are recognised in their fundamental importance.<sup>24</sup> This is the reason why Canguilhem normatively rejected quantitative conceptions of pathology. Quantitative versions of pathology aims to fully objectify illness, that is, to reduce it to quantitative variations which can be dominated and manipulated by the practitioner. In these views, the patient becomes a mere object and does not play a role in the shaping and progress of the discipline.

## 7. Conclusion

Are the norms in the life sciences as presented in *Le normale et le pathologique* and the norms of his history of science the same? Normative history as Canguilhem practised it in *La formation du concept de réflexe* does not suggest that the norms which Canguilhem employed to judge past science came from ‘life’, as did those described in *Le normale et le pathologique*. Canguilhem based his history of the concept of reflex on the current scientific concept and employed ‘logic’ rather than strictly historical considerations in order to carry out his evaluation of the links between past and present concepts of reflex. In *La formation du concept de réflexe* the norm comes from current science, as its aim is to judge which past concept is consistent with the modern one. However, in *Le normale et le pathologique*, Canguilhem aimed to show that norms on which science is based are instituted by ‘life’. The norms employed by Canguilhem in his normative history are therefore directly those of science, but indirectly those that social and biological life creates.

Nonetheless, the concept of norm that Canguilhem defended in his book does not belong to science in the same way in which the concept of reflex does. Whereas the concept of reflex can be scientifically defined, the concepts of norm, normality and pathology are, so to speak, regulative ideals that guide the life sciences. Canguilhem was of course well aware of this, and indeed concluded *Le normale et le pathologique* by saying that the concept of norm cannot be scientifically established. Similarly,

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<sup>24</sup> Canguilhem’s view of the concept of normal in medicine and psychology explains his participation in a conference that, in the words of its organizer, was prompted by the need to discuss a reform of the national health service in Italy (Manuali, 1988, p. 7); Canguilhem’s paper was on ‘Le statut épistémologique de la médecine’ (Canguilhem, 1988a). This conference, titled ‘Medicine and Epistemology. Health, Disease and Transformation of Knowledge’ was held in Perugia (Italy) in 1985 and chaired by the psychiatrist Carlo Manuali. In the mid-Sixties, Perugia’s psychiatric institutions participated in one of the pioneering applications of ideas coming from the new psychiatric movement, aimed at the de-institutionalization of the mentally ill and at a re-definition of mental illness. The director of this experiment—which was going to find a more widespread application in the Italian reform of the laws governing psychiatric care in 1978—was Manuali himself. In his Introduction to the Perugia conference, Manuali declared that ‘the new legislation envisaged an improved service that was more equitable, more accessible and more controllable by the community. Moreover, its aim was to promote a collective health culture that would counteract the tendency in sanitary policy to ‘medicalize’ increasingly large parts of individual behaviour and experience . . . The greatest obstacles to the reform, or at least the obstacles that I shall discuss here, lie in medicine itself. They are inherent to its epistemological structure and related to the ways in which medical knowledge defines its subject, and organizes and transmits its knowledge’ (Manuali, 1988, p. 7).

he argued that there could be no objective method to determine what counted as disease, as this would always depend on the subjective experience of the sick person. If this is the case, and indeed I think that Canguilhem is correct, he is implicitly vindicating a role for philosophy that is quite independent from science. Philosophy appears as a discipline capable of studying the space and relation between life and science. Canguilhem aimed to show that the concepts, and indeed the *raison d'être*, of science are produced by life as a whole, and not only by rationality, as Bachelard wanted.<sup>25</sup> The relation between life and science can only be analysed by philosophy, which can apply its critique to both. At a general level, Canguilhem retained the traditional role of philosophy (at least since Kant) as critique, but his is an open-ended critique. This for two reasons: because of the historical character of his epistemology, and because of his type of rationalism, which is very similar to Bachelard's. Their rationalism does not aim to close discussions and to solve problems once and for all. Rather, Canguilhem often problematized what may have been assumed as unproblematic, for instance, what disease or health are. He claimed that the aim of philosophy was that of making life 'more complicated' rather than simpler:

The author maintains that the proper function of philosophy is that of making human existence more complicated, including the existence of the historian of the sciences. (Canguilhem, 1993 [1977], p. 139)

But can history of science complicate the philosopher's life? My view is that, in order to develop his philosophical theses, Canguilhem kept history simple. In his work, history is at the service of philosophy: he judges a selection of past concepts, without presenting them in a historical context. Canguilhem evaluates Willis' concept of reflex and Descartes' concept of involuntary movement in relation to their general physiological theories, but he does not explain which cultural values support them, on how these values came about. Canguilhem approvingly cites Henry Ernest Sigerist who said that all changes in the medical culture have been conditioned by the changes in contemporary ideas. (Canguilhem, 1999 [1966], p. 61). However, Canguilhem does not offer a historical treatment of the society and culture in which the ideas that he examined developed, and he does not link these ideas to any place in particular. In other words, he does explain the origins of current norms, but does not give an account of the formation of past norms. His interest lies in the present, not in the past.

Canguilhem argues that rationality does not come all 'from above'; indeed he argues that the rationality of experts—of scientists and technicians—depends on rationality 'from below', on the rationality of those who are directly affected by the advances in science. He cites the example of organ transplantation: this practice

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<sup>25</sup> Canguilhem only referred to medicine and life sciences rather than all sciences. However, I think that his argument could be extended to the so-called exact sciences. Certainly their existence depends on social values attached to scientific research, and indeed its technological uses.

presupposes a ‘general indifference’ to the problem of the identity of individuals with the whole of their organism. (Canguilhem, 1994 [1968], p. 401). This thesis of the rationality ‘from below’ is affirmed, implicitly or explicitly, in large part of his work, and especially in *Le normal et la pathologique*. He argues that the creation of norms and values is not the reserve of the experts, but it is rather a complex process in which every aspect of life—biological, social, cultural, political and institutional—contribute.

Canguilhem’s view of the formation of scientific concepts is, however, not simply descriptive but normative. The most important examples of this is his concept of illness. He thought that what constitutes an illness should be founded on the subjective experiences of patients, which in turn depend on biological, social and psychological factors. In his convincing account, this, however, has not always be the case. In a talk in which his appreciation of Foucault’s study on the birth of the clinic is apparent, Canguilhem recognized that there has been a ‘progressive elimination’ of the patient’s life circumstances from the knowledge of illnesses. He indicates two roots of this trend. The more apparent, but maybe not the more important, is for him the ‘colonization’ of medicine by the ‘fundamental sciences’. The second is the characteristic link between politics and medicine that has existed in industrial societies from the beginning of the nineteenth century onwards. In these societies, he argues, a keen attention is given to the health of working people as productive forces, rather, one could add, than as complete human beings. Political authorities, Canguilhem continues, establish rules and procedures in order to improve and control people’s life conditions. A major expression of this aim to control and discipline people’s health and lives is the hospital. Another expression, in Canguilhem’s view, is the rise in prestige of practitioners of specialized branches of medicine and the decline of that of general practitioners. The former do not focus on individuals as wholes but rather on specific parts or aspects of their bodies (Canguilhem, 2002 [1989]).

Canguilhem’s holistic view of how norms come to be established may be the foundation for a revision of the epistemological break between science and opinion. In *Le normal et la pathologique*, scientific norms are not constituted against opinion, or against every-day desires and impulses, as Bachelard maintained. However, Canguilhem did not aim to eliminate the epistemological break: for him there is science and there is non-science. Unlike Bachelard, though, the difference between the two is not determined by the presence or absence of subjectivity and extra-scientific values and aims. For Canguilhem, science, or at least medicine and life sciences, are always in a continuum with values and aims that are not produced by science. However, some of these are legitimate, while others are not. The normativity of Canguilhem’s project is expressed in the judgement of values and of their provenience. For instance, the individual’s subjective perception of his or her health should be the basis upon which medicine operates. This perception, which varies according to specific cultures, societies and circumstances, becomes part of medicine itself. For Canguilhem, other values and aims should stay extra-scientific. The aim to control and discipline people’s lives is not for him an acceptable value, and must be expelled from the organization of medicine.



In his article ‘Qu’est-ce qu’une idéologie scientifique?’ (Canguilhem, 1993 [1977], pp. 33–45), Canguilhem implicitly proposes two epistemological breaks. The first is that between pre-science and science, along Bachelard’s lines, that is, where pre-science is a false science. The other is between science and ideology, which is a sort of ‘post-science’ (Canguilhem does not employ this term). Canguilhem’s ‘scientific ideology’ is post-science because it is parasitic on an existing science. He cites the example of Herbert Spencer’s evolutionism. He observes that Spencer claimed to have based his theory on Karl-Ernst von Baer’s embryological research and to have had it confirmed in Charles Darwin’s *Origin of Species*. Canguilhem argues that van Baer’s biology and Darwin’s theory lent scientific credibility to a project of ‘legitimation of free enterprise, [and] of the correspondent political individualism and competition’. Here Canguilhem’s concept of scientific ideology is close to Karl Marx’s classic concept of ideology in that for Canguilhem ‘evolutionistic ideology works as a self-justification of the interests of a type of society’, although he does not bring in class conflict (Canguilhem, 1993 [1977], p. 43). It is clear that for Canguilhem the justification of the interests of the industrial society cannot and must not become part of science.

In Canguilhem’s work, however, we do not have any historical, or sociological, study of the formation of rationality ‘from below’, or of how scientific concepts are distorted ‘from above’. *La formation du concept de réflexe* is limited to the examination of the work of philosophers and scientists. *Le normal et la pathologique* is a defence of formation ‘from below’ of the norms of medicine and psychiatry. Canguilhem aims to bring the patient back not simply into the process of decision-making as far as her own health is concerned, but also into the broader formation of the values and norms that form the very basis of medical knowledge. He chooses, though, to conduct this defence in philosophical terms, and within ideal discussions with other philosophers and physicians. Only the experts are embodied in Canguilhem’s account; the producers of the rationality ‘from below’ have neither history, nor social and cultural identities, not to mention personal ones. They are ‘patients’, ‘organisms’ or even an expression of ‘life’.

The fact that Canguilhem did not substantiate his theses historically does not detract, however, from their philosophical value. Indeed, some of his ideas and intuitions can still serve history and philosophy rather well. Precisely the study of the dynamics ‘from below’ of the formation of norms, values and concepts that become part of disciplines, seems to me of particular interest. However, this study, in order to have real strength, would need to recuperate what historical epistemology dropped: if not an actual total history, a commitment to an exhaustive historical and sociological substantiation of any theory of formation of norms and concepts. In other words, ideas should be re-embodied. Careful analysis of historical settings and events would also prevent implausible generalizations and the risk of attributing ideas to entire societies or historical periods without investigating their actual emergence and roles. In science studies the reconnection of history and society with knowledge has been pursued in many different manners and from many different angles. However, philosophy seems to be preserved from the attacks of contingency, of historical



events and persons.<sup>26</sup> The reflexivity that sociologists like Pierre Bourdieu have brought to their own discipline would inject new life into philosophy as well. As a result, we could also treat philosophical concepts as objects of study, and analyse their formation ‘from below’ as Canguilhem has taught us.

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<sup>26</sup> Recently, some science studies scholars have engaged in this type of enterprise. See Kusch (1995, 2000) and Collins (2002).

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